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Editorial discretion: Peer review or publish and perish?

The orthopaedic and general scientific world is entirely awash with publications and their associated data. I recently came across, and was surprised by, a relatively recent paper from the journal *eLife*, in which researchers established that synapses are more elaborate than previously thought.[1] Their research puts the storage capacity of the human brain in the 'petabyte' range – more than the volume of information available on the entire internet. That, of course, is not to say that we can process, understand, or even learn this volume of information, but it does highlight the intricacy of biological systems. These days, acquiring data is cheap, and the internet is growing exponentially in size and complexity. This is reflected everywhere, and can clearly be seen in medical science. The volume of papers in indexed and unindexed journals is rising day on day. We are in the middle of an information explosion, and I would like to say that this is for the good, but I fear that the vast majority of what is published now is irrelevant, potentially inaccurate, and often of low quality, leaving the reader at a loss to know what to read. How did we get here? Surely with the much relied upon and lauded peer review process (a system that would, of course, not withstand any form of scientific scrutiny itself), all that is published must be worthy of reading? Well, perhaps not quite; the history of the peer review process is as interesting as its failings.

In the earliest days of scientific endeavour, the first journals were often 'proceedings' of societies, simply reports or letters of the discussions had at the various academic institutions that published them for wider dissemination. The first two widely recognised scientific journals are generally accepted to have appeared in 1665: in France, the *Journal des sçavans*; and the English *Philosophical Transactions of the Royal Society*. Both published research results and were distributed to an interested and scientific readership.

The concept of 'peer review' as a process appeared later in the history of academic endeavour, and, although far from clear, it appears that the Scots took the initiative. *Medical Essays and Observations*, published by the Royal Medical Society of Edinburgh initially in 1731, was likely peer-reviewed, although at this stage it was an internal process. By the mid-19th century, external peer review was becoming the norm, but not necessarily popular. Albert Einstein's initial four revolutionary *Annus Mirabilis* papers were published in the 1905 issue of *Annalen der Physik* and were peer-reviewed by the journal's editor-in-chief, Max Planck, and co-editor, Wilhelm Wien (each of whom went on to win a Nobel prize). However, following the introduction of external peer review, Einstein took a position on this process that perhaps we would all like to take on occasion, saying that he had not authorised the sharing of his manuscript "to specialists before it is printed", and he therefore decided he would "publish the paper elsewhere".

The question I would now ask, in the information age of petabytes and exploding numbers of papers available, is whether the peer review process is still fit for purpose. There have been a number of high-profile failures of the peer review process, including truly seminal work that has subsequently won a Nobel prize. Had the original reviewers had their own way, we would have had to do without Hans Krebs' description of the citric acid cycle, Solomon Berson's discovery of radioimmunoassay, and Bruce Glick's identification of B lymphocytes, all of which, remarkably, were rejected at peer review.

The problem stems from three intrinsic weaknesses of the process: the reviewers may well not be experts in the field, and yet will often still offer a review; reviewers always have a vested interest and this may or may not match the opinion of the authors of the papers; and with truly remarkable and game-changing science, where a paradigm shift in thinking is called for by the authors, by definition this will be unpalatable to external reviewers.

As the number of publications mushrooms, so does the number of submissions, and the number of reviewers needed. We are perhaps in danger of reaching a perfect storm where there are so many papers being published (many in journals of dubious quality), and such increasing demands on the many reviewers who do an excellent and unbiased job, that there is a growing risk of throwing out the wheat with the chaff.

REFERENCE

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